more highly specialised than in any existing flowers whatever; and if we take into account the world-wide distribution of these plants, their intense richness in genera and species, and their wonderful complexity of structure, we must consider them as among the most ancient instead of the most recent of flowers."

Without venturing any opinion as to the geological age to which the development of this wonderful order of plants must be traced back, it seems to me that there are some classes of facts concerning our native European species which support the conclusion that their existence in their present specific characters must date from a very remote time.

1. It is an important fact that out of fifty species of orchids enumerated in Garcke's "Flora des deutschen Reiches" (exclusive of the Bavarian Alps, which possess two or three more species), not less than forty-one occur in the British Isles (besides *Neotinea intacta* and *Spiranthes romanzoviana*, not found in Germany), a proportion considerably exceeding that of phanerogams generally. Now, as it seems scarcely credible that orchids should possess means of transportation across the sea in preference to other plants, we must conclude that they inhabited the British Isles before their separation from the Continent, which involves that they occupied stations near the present coasts of Germany or France previous to a great deal of plants that reached these coasts only subsequently to the formation of the Channel. These conclusions are rather strengthened by the fact that several orchids are by no means frequent in Germany, and very rare and local in Britain, which proves that their occurrence is not to be accounted for by favourable present conditions, and even renders it probable enough that some of the species found in Germany, but not in Britain, may in the latter country have become extinct in times not very long ago.

2. Notwithstanding the light so plentifully thrown on the significance of the floral peculiarities of our orchids by Mr. Darwin's admirable investigations, there remain some species whose relations to insects, although evidently of a most specialised nature, are yet very little understood. Such seems to be the case with Himantoglossum and with Ophrys. It is therefore to be suspected that the adaptations of these species may point to insects no longer existing in our countries. However, I should not insist on this point were it not somewhat connected with the following:—

3. It has been observed by Mr. Darwin that "the frequency with which throughout the world members of various orchideaceous tribes fail to have their flowers fertilised, though they are excellently constructed for cross-fertilisation, is a remarkable fact." And further on Mr. Darwin alludes to the unknown causes which lead to the destruction of seeds or seedlings, forming a check to the multiplication of orchids. Indeed, with many of our native species, though abundantly fertilised, multiplication by seeds is evidently but a rare exception to the general rule of propagation by side-bulbs, *i.e.*, mere individual persistence. Thus the wonderful contrivances for cross-fertilisation point back to different conditions of life in the past, under which their function must have been much more active and important than it is now.

There may, I think, be found analogous cases in very different quarters of the vegetable kingdom; for instance, the frequent reduction of the peristome of mosses to mere rudiments is probably connected with actual preponderance of vegetative propagation over propagation by spores. In the orchids, too, there are already perceptible some traces of a regressive change of the apparatus for cross-fertilisation (for instance, in *Ophrys apifera*), as will be inevitable in the course of ages, whenever specialised structures are no longer sustained by active function, leading to their reproduction under the agency of natural selection. Perhaps such regressive change goes on more slowly in cases of merely vegetative propagation.

Finally, I may allude to the fact of our native orchids belonging to very different groups of the order, and this enhances the argument for antiquity, based on their geographical distribution.

D. WETTERHAN

Freiburg im Breisgau, May 10

Barometric Pressure and Temperature in India

IN order to satisfy myself as to whether some of Mr. Broun's conclusions (NATURE, vol. xix. p. 6) held good when inland stations were included in the comparison, I recently examined in detail the pressure and temperature oscillations at fifty-one stations in the Indian peninsula, computed from the means given in Mr. Blanford's "Indian Meteorologist's *Vade Mecum*." As these stations represent every part of the country, the results afford a basis for deduction of sufficient extent to be reliable.

An inspection of these shows (1) that the range of pressure-oscillation corresponding to 1° F. *varies* very much at different places, its extreme limits being 0·002 in. at Leh, and 0·032 at Vizagapatam, and its mean value for all the stations 0·017 in.

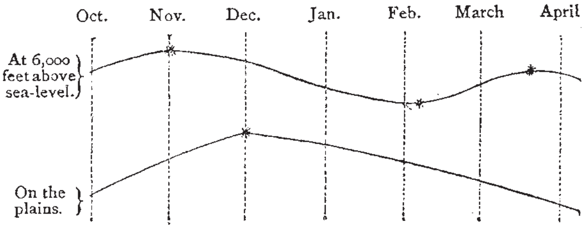
2. That on arranging the stations in order of elevation, it is plain that this variation is partly related to the height above sea-level, being least at Leh (11·538 feet), the most elevated station, and increasing from thence downwards at the mean rate of about 0·002 in. for every thousand feet of descent. The numerous deviations from this rule, however, especially at the lower elevations, make it evident that other factors operate besides height, such as distance from the coast and latitude. Though no rigorous comparison with these has as yet been made, a mere inspection of the results is enough to show that proximity to the sea, and, therefore, *ceteris paribus*, greater humidity is associated with a greater barometric oscillation corresponding to 1° F.[1]

3. That when the element of horizontal space alone is considered, the pressure oscillation increases with the temperature oscillation, but not very regularly, and that while this relation holds pretty generally in the plains, it disappears altogether at the more elevated stations, or, when the element of vertical space is introduced.

Now while the pressure ranges generally decrease pretty regularly as we ascend, the temperature-range remains about the same at different heights. The decrease, therefore, in the pressure-oscillation for 1° F., noticed in the case of elevated stations, must plainly be solely due to the contraction which takes place in the pressure-range.

The diminished mass of atmosphere above the more lofty stations, doubtless in a great measure accounts for this diminished amplitude of the oscillation. Another important factor, however, in this result, must not be overlooked, viz., the *double* annual oscillation of monthly mean pressure which takes place at stations of 6,000 feet and upwards in height, apparently throughout India, the effect of which is sensibly to diminish the absolute annual range of pressure-oscillation at these heights.

In fact, the curves representing the mean monthly barometric pressure in the winter at the hills, and on the plains respectively, may be taken to approximate more or less in character to those in the adjoining figure.



Pressure Curve (the critical points are marked with asterisks).

Now as the pressure on the plains is made up of the pressure at a height of 6,000 feet + the weight of the stratum of air between, it is evident that the latter must reach its maximum somewhat later than December. Moreover, as the winter depression at the highest stations is considered to be caused by the inrush at their level of the saturated anti-monsoon at this season,[2] the direct effect of the cold of this season in increasing the density, and hence the pressure of the atmosphere, will only be felt in the stratum of air between them and the plains. It is im-

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[1] This relation appears to be independent of that due to elevation, with which it might be thought identical, owing to the general decrease in the heights as we travel seawards. The following stations which have nearly the same elevation, but are at widely different distances from the sea, will show this very clearly:—

	Height above sea in feet.	Bar oscillation for 1° F.
Lucknow	369	0·017
Sibsagar	332	0·028
Difference ...	37	0·011

Now if the approximate rule for difference of elevation already mentioned, viz., a decrease of 0·002 inch in the bar-oscillation for 1° F. for every 1,000 feet of ascent be applied in this case, the decrease at Lucknow should evidently be inappreciable. As it is, however, it is of very considerable magnitude.

[2] *Vide* "Indian Meteorologist's *Vade Mecum*," by H. F. Blanford, p. 75.

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portant, therefore, to find out when the maximum average density, or what is practically the same thing, the maximum barometric weight of this stratum, occurs, and more especially to see whether it coincides with the epoch of minimum temperature, which, as a rule, occurs in January throughout India.

The following table, in which I have calculated the mean monthly barometric weight of such a stratum in different part of the country, will show that the maximum invariably occurs in January, that is to say, it *coincides* with the epoch of minimum temperature. Two bars placed underneath a figure indicate the maximum pressure or weight of the year; one bar, the secondary maximum at the hill-stations in the spring.[1]

Stations.	Elevation above sea-level in feet.		Mean monthly barometric pressure in inches. October.	November.	December.	January.	February.	March.	April.
Leh	11,538		19·714	·707 *	$\underline{\underline{\text{·728}}}$	·553	·571	$\underline{\text{·656}}$	·630
Lahore	732		29·144	·262	$\underline{\underline{\text{·332}}}$	·243	·237	·107	28·962
Stratum between—thickness	10,806	weight	9·430	·555	·604	$\underline{\underline{\text{·690}}}$	·666	·451	·332
Chakrata	7,052		23·332	$\underline{\underline{\text{·356}}}$	·352	·304	·313	$\underline{\text{·325}}$	·309
Roorkee	887		28·965	29·102	$\underline{\underline{\text{·150}}}$	·108	·050	28·965	·853
Stratum between—thickness	6,165	weight	5·633	·746	·798	$\underline{\underline{\text{·804}}}$	·737	·640	·544
Darjeeling	6,912		23·436	$\underline{\underline{\text{·472}}}$	·449	·382	·368	·364	·363
Goalpara	386		29·461	·593	$\underline{\underline{\text{·641}}}$	·610	·544	·459	·383
Stratum between—thickness	6,526	weight	6·025	·121	·192	$\underline{\underline{\text{·228}}}$	·176	·095	·020
Ranikhet	6,069		24·108	$\underline{\underline{\text{·180}}}$	·158	·079	·070	·055	$\underline{\text{·070}}$
Lucknow	369		29·503	·651	$\underline{\underline{\text{·696}}}$	·641	·596	·481	·348
Stratum between—thickness	5,700	weight	5·395	·471	·538	$\underline{\underline{\text{·562}}}$	·526	·426	·278
Wellington	6,200		24·217	·245	$\underline{\underline{\text{·256}}}$	·208	·226	·246	$\underline{\text{·247}}$
Madras	22		29·847	·922	$\underline{\underline{\text{·965}}}$	·944	·921	·895	·843
Stratum between—thickness	6,178	weight	5·630	·677	·709	$\underline{\underline{\text{·736}}}$	·695	·649	·596

* To avoid repetition, I have merely given the decimals after the first column. In every instance of omission the last prefixed integer is to be supplied.

Without going any deeper into the matter, it must, I think, be generally admitted that the preceding facts not only dispose of Mr. Broun's objection to the idea that pressure and temperature are related, because the epoch of maximum atmospheric pressure on the plains of India generally *precedes* that of minimum temperature, but also show how abortive any attempt to base inductions regarding a secular variation in solar heat, upon the results of comparing the annual range of monthly mean pressure, or even the mean annual pressure, for a number of years in succession in different parts of India, must necessarily prove, unless they be duly taken into consideration.

With reference to Mr. Broun's conclusions alone, the following modifications should be attached, according to the results of my investigation.

1. The annual oscillations of monthly mean pressure and monthly mean temperature bear an exceedingly *variable* ratio to one another in India, such variation being a function partly of the altitude, and partly of the distance from the coast.

2. Non-coincidence of the critical epochs of monthly mean pressure and temperature, cannot be rigorously employed as an argument against the hypothesis, hitherto generally accepted, of a causal connection between them.

E. DOUGLAS ARCHIBALD

Insect Galls Buds

INSECT galls are held to be "excrescences"; a "diseased condition of vegetable tissue"; and they are supposed to result from the "injection of a fluid," or from some "secretion." The student may most easily begin an investigation of galls with the dissection of those produced by the turnip-weevil (*Curculio pleurostigma*) on the bulb of the Swede. The roots of Swedish turnips are frequently covered with hundreds of irregular spherical warts, from ·03 to ·75 of an inch in diameter, growing either singly or crowded together in clusters. These warts are regarded by M. Woronin (*Plasmodiophora brassicæ*, Pringsheim's *Jahrb.* xi. B. p. 548) as resulting from the fungus which he has discovered to be the cause of club-rooting in cruciferous plants. I believe that on this point M. Woronin has been misled. The true clubs produced by his fungus are entirely distinct from these root-nodes. Under favourable conditions the root-nodes have been found to give rise to tufts of leaves; a fact which I can confirm by many examples presently growing in my possession. Dissection of these nodes, on Swedes, shows that they contain none of the plasma and spores which constitute the bulk of the true clubs. They are, in fact, tuberculated buds arising directly from medullary rays in the root to which they are attached. These can be traced through the enveloping parenchyma into the nodes, where they are seen to give rise to masses of contorted and branching leaves. The nodules within the bark of the beech, hazel, and other trees, are of the same character as those on the turnip. The medullary nexus of these nodules sometimes comes straight from the centre of the tree into the node, and sometimes runs along like a cord under the outer layers of the bark, entering the node by the end.

Let a dissection now be made of one of the weevil galls on the bulb of the turnip. The second or third slice will show the outer foliations, exactly similar to those of the root buds. When the centre has been reached, where the maggot will be found, there will also be found a vascular pencil running up from a medullary ray in the bulb, and bearing on its top a bud of the same description as that produced by a ray running out from a root. The insertion of the ovipositor brings a medullary ray into action, producing a tuberculated bud, and it is only the bud which the larva feeds upon. The growth of a bud is an intelligible cause of the growth of a gall, but we can infer nothing from the injection of a fluid.

All insect galls are in reality leaf-buds, or fruit-buds. They are not mere amorphous excrescences. The vascular lines which would form leaves can easily be followed up the structure of the oak-leaf galls. And in cases where the egg has been deposited in the tissue of a young branch the cap of the gall is sometimes surmounted by a leaf two or three inches long. But in the large blue Turkish galls many lacunæ occur where the fleshified leaves have not filled up the spaces between them. The morphology of the hollow woody shell, and its inclosures of starch, &c., found in the interior of these galls I hope to work out by and by. It is a curious fact that various microscopic fungi are matured in the interior of imperforate galls.

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[1] In selecting the particular station on the plains to be used in each case, I have endeavoured as far as possible to fulfil two requisites: (1) proximity to the hill-station, (2) low elevation above sea-level.

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